MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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WARREN DE LA RUE, Esq., Vice-President, in the Chair.

Lieut. W. H. Collins, R.E.;
Rev. John Edwards; and
Rev. Alphonso Matthey,

were balloted for and duly elected Fellows of the Society.

Solar Eclipse of August 7th, 1869.

By R. T. Paine.

This eclipse was carefully observed by me at the Court House in Boonesboro, Boone County, Iowa, with my small telescope of $2\frac{1}{4}$-in. aperture, power of 30, erect eye-piece, deep green screen. The latitude, 42° 3′·4, was determined by me, by a few altitudes of *Altair*, which agreed well with each other, but *Polaris* was too faint to be used. The longitude was ascertained by my two chronometers, by comparison with the clock of the Observatory at Chicago, to be $6^h\ 15^m\ 34^s{\cdot}97$; $25^m\ 7^s{\cdot}64$ going to Boone, and $9^s{\cdot}0$ returning from it to Chicago, which is in long. $5^h\ 5^m\ 26^s{\cdot}65$ west from Greenwich. Both latitude and longitude of Boone are, therefore, considered as pretty well known.

The observations were good and the sky clear, although there was some haze.

The corona was good, but, independently of the red or rosy

flames, not, I think, as striking or magnificent as the one at Beaufort, S.C., on November 30th, 1834; moreover, the darkness was *not* as great. I used a lantern on both occasions to read off the chronometers at the second and third contacts, but it was *not* necessary on August 7th. As soon as the second contact took place at Boone the corona was seen, and then a *deep red* flame, a little to the left of the lowest part of the Moon, which remained quite steady to the third; two or three seconds later, two flames of a beautiful rose colour appeared on the left and upper part of the Moon, and continued upwards of a minute: as the Moon moved on, three or four *rosy* flames appeared on the right, which, just before the third contact, seemed to run together. About five minutes before the second, a brush of light descended from the upper crescent of the Sun, and at the *same time* a small part of the lower crescent was broken off, yet nothing of the kind was noticed after the third. The edge of the Moon was generally very sharply defined, but slight irregularities were seen before the second contact, of very short continuance. The red or rosy flames were so conspicuous that they were seen at once, without the assistance of even an opera-glass; but at Beaufort nothing of the kind was noticed, although α *Scorpii*, then (Nov. 30th) in conjunction with the Sun, and therefore only $4\frac{1}{3}^\circ$ distant, was seen as soon as the corona. This eclipse will return for the second time on December 22nd, 1870, total at Cadiz, Gibraltar, Syracuse, &c. At Beaufort, lat. 32° 25′ 57″, long. 80° 40′, the interval between the second and third contacts was $1^h\ 49^s{\cdot}6$, and the eclipse was quite central.

The total eclipse of June 16th, 1806, which occurred *here*, central, about half-an-hour before noon, was the finest in the United States in the nineteenth century. The duration of totality at Boston and at our neighbouring city, Salem (where it was observed with great care by the late Dr. Bowditch), was *five* minutes. The excitement about it was great indeed, yet no one saw any red flames, and Dr. Bowditch never gave the least hint that he saw any; and a very intelligent gentleman of Lynnfield, who saw that eclipse in that town, and distinctly recollects the phenomena attending it, and the last eclipse at Springfield, Illinois, tells me he is positive the darkness in June 1806 was much deeper, and that there were no flames, as he certainly must have seen them, as he easily did at Springfield. Yet at the second and third returns of the eclipse of 1806, in 1842, and 1860, these flames were generally observed. The fourth return on July 29th, 1878, will be central in the United States, in Colorado, Texas, &c., and will therefore, doubtless, be carefully observed by even a greater number than that of August last, although then the number of observers on the central line, or very near it, was not small.

The phases of the eclipse of August 7th, 1869, at Boonesboro, Iowa, lat. 42° 3′ 23″, long. 93° 53′ 45″, were as follows, by strict computation and observation: —

	By computation. h m s	By observation (Mean of two Chrons.) h m s
Beginning of Eclipse	3 40 37·6 M.T.	3 40 46·78
,, Totality	4 43 20·6	4 43 24·48 = +3·88
End of Totality	46 20·6	46 23·28 = +2·68
,, Eclipse	5 43 36·5	5 43 30·18
Duration of Totality	3 0·0	2 58·8

Boston, Oct. 2nd, 1869.

Solar Eclipse of August 7th, 1869. By E. D. Ashe, Commander R.N.

(*Extract from a Letter to Mr. De La Rue, dated Observatory, Quebec, August* 28, 1869.)

I hasten to communicate to you the result of the Canadian eclipse party that went to Jefferson, Iowa. It consisted of Mr. Douglas, Mr. Falconer, and myself. The funds would only admit of my taking the equatoreal of 8-inch aperture, 9 feet focus, therefore I leave to the American astronomers the description of the results obtained by the spectroscope, &c.

Jefferson is a little to the south of the central line of eclipse. The totality was a few seconds over three minutes. The morning was cloudy; about noon the Sun appeared, and although there were no clouds during the eclipse, still it was hazy.

The total eclipse began at $4^h\ 41^m\ 6^s$, so the Sun was at a very good altitude.

I took several photographs of the partial eclipse, enlarged by an eye-piece to $3\frac{1}{4}$ inches; one shows distinctly a band of light round the limit of the Moon on the Sun. I was afraid of the hazy atmosphere, and altered my arrangements for taking photographs during totality with the eye-piece, and put in the tube fitted for taking the Sun in the principal focus; and had I not done so, the actinic power was so bad that I should have failed completely. In consequence of the change I had no wires to connect the protuberances with the axis of the Sun; but this is of no consequence, as the American party fifty miles to the southward have well determined their position.

I took four negatives* during totality with an exposure of 10^s, and when they are examined with a magnifying glass they are full of information.

* These are now in Mr. De La Rue's care.—Ed.

American Photographs of Total Solar Eclipse of August 7, 1869.

By Rev. T. W. Webb.

Through the courteous intervention of my valued correspondent W. S. Gilman, Jun. Esq. of New York (himself an accomplished observer of astronomical phenomena), I have been favoured by Professor Mayer, of Lehigh University, Pennsylvania, with some specimens of the photographs taken by him during the late total Solar Eclipse of August 7. As these were sent direct from America to Somerset House, I have not had an opportunity of seeing them; but, as I have every reason to believe that they will prove acceptable to the Society, I beg permission to forward to you the following explanatory statement, taken, with some abridgment, from Professor Mayer's letter to myself:—

"The Photographic Expedition, of which these photographs are some of the results, was organized by Prof. Henry Morton, Secretary of the Franklin Institute, Philadelphia, under the authority of Prof. I. H. C. Coffin, U.S.N., the Superintendent of our National Nautical Almanac. The expedition was divided into three parties, stationed respectively at Burlington, Mount Pleasant, and Ottumwa, in the State of Iowa. To me was assigned the organization and command of the station of Burlington, and the photographs I send were taken by me at that place.

Burlington, Iowa, is a town on the W. bank of the Mississippi River, situate in Lat. N. 40° 48′ 21″·58, Long. $0^h\ 56^m\ 13^s{\cdot}88$ W. of the Observatory at Washington. This station was about 7 miles N. of the centre of the Moon's shadow.

"The telescope, by Merz and Mähler, of 6·42 inches aperture and 9 feet focus, was equatorially mounted, and driven by one of Frauenhofer's friction-governor clocks. The Sun's image, 2·04 inches in diameter, was formed on the plate of the camera by a negative eye-piece specially calculated to give the least aberration. The image of a reticule of two spider-threads at right angles was also projected on the plate with the Sun's disk, and one of these lines was accurately adjusted to the celestial equator; and thus the photographs give precise position-angles of the contacts and of the protuberances.

"A plate having a slot of $\frac{1}{20}$ inch in breadth shot across the eye-piece by the action of a spring for the exposure during partial phase. The duration of this *flash* of the Sun upon the camera-plate I have made the subject of experimental investigation; and I find it to have been almost exactly $\frac{1}{300}$th of a second. A 2-inch aperture of object-glass was used during partial phase work. During totality the full aperture was used, and a slide which allowed the whole beam to fall upon the plate; the exposures varying from 5 to 7 seconds.

"Forty-one perfect photographs were taken during the eclipse, and five of these (all of which I send) were taken during totality, which lasted with us $2^m\ 42^s$. I send nine plates taken at the

times I place opposite the numbers, which correspond with those on the plates.

	No.	Sidereal Time of Burlington. h m s
Before Totality	4	13 2 4·1
	19	13 49 39·8
	21	13 58 32·4
Totality	23 (Exposure 5ˢ)	14 4 1·4
	24 („ 5)	14 4 29·6
	25 („ 7)	14 4 52·8
	26 („ 7)	14 5 10·37
	27 („ 7)	14 5 40·5
After Totality	35	14 34 59·5

" The times were electrically recorded on a chronograph by the exposing plate breaking the electric circuit.

" The photograph numbered 4, taken 2ˢ·8 after *observed* contact, shows a depression in the Sun's limb at the position of first contact, and from this depression shoots into the Sun a high lunar mountain, whose position, measured from the S. point of the cusps, is $\frac{1}{4}$ of the distance to the N. point of the same.

" You will observe how beautifully defined are the two large spots in the S.W. and N.E. quadrants; the latter surrounded by well-developed faculæ, one of which seems to bridge over the spot and divide it into two portions. I also call attention to the gradation of shade from the border of the Sun inward, the faculæ, the mountains on the Moon's limb, and a glow like that of early dawn (also obtained in the photographs by Mr. De La Rue in 1860), which extends to 18″ beyond the limb of the Moon.

" I could hardly have wished for better success during totality. I might probably have obtained more of the corona by longer exposures, but it would have been at the expense of the fine definition in the prominences, to which my work was specially directed.

" Will you have the courtesy, my dear Sir, to present to the Astronomical Society the nine photographs on glass taken from the original negatives with an orthoscopic lens: I wish them to be preserved in their collection. The other three enlarged copies on paper (also made with an orthoscopic lens) I wish you to receive as a present from me. The best way to examine the glass photographs is to incline them over a piece of white drawing paper placed before a north window, and use a lens magnifying about eight diameters."

Thus far, in substance, Professor Mayer, who has also kindly offered for the Society's acceptance a copy of his Report on the Eclipse, published in the Journal of the Franklin Institute, which contains many additional particulars of much interest. He has also presented to them a copy of his Lecture-Notes on Physics.

Hardwick Vicarage, Nov. 10, 1869.

The November Star-Shower in 1869. By A. S. Herschel, Esq.

The observations of the appearances of the November Meteors, during the last few years of their great returns, present certain irregularities, which it is either possible for physical astronomy to explain, by calculating the perturbations exercised upon the course of the stream by the attraction of the planets, or which may arise from the peculiar configuration of the meteoric current. Supposing the main body of the November meteors, which, in the years 1866 and 1867, made its appearance, successively, in Europe and America, to be an individual portion of the stream, subject during the years 1868 and 1869 to a continuance of the same perturbations which made its return in the year 1867 take place two hours later than the time when (from its occurrence in 1866) it was expected to appear; supposing also that the density of the current, at the point where the Earth will traverse it in the present month, is not yet exhausted,—the time of reappearance of the main-stream of meteors, this year, in England will be almost exactly the same,—between 1^h and 2^h A.M. on the morning of the 14th instant,—as that of its greatest intensity in 1866. A watch for meteors continued from the time of *Leo's* rising on Saturday night the 13th, until sunrise on Sunday morning next, the 14th instant, will probably be rewarded by a view of the return of the central portion of the meteor-stream in Great Britain, although, doubtless, owing to the waning character of the phenomenon, with diminished intensity, yet under the same favourable conditions as those in which it was observed in England in 1866.

Observations at other times and places than those of the great returns of the star-shower in 1866 and 1867 appear to indicate the existence of two meteoric currents bordering upon the main-stream, but separated from it by blank spaces almost entirely devoid of meteors, and forming lateral outliers of the stream, near to which they appear to move in parallel, and closely adjacent orbits. The passage of the Earth through the first of these outlying streams occurs about twelve hours earlier, and its passage through the last about twelve or fifteen hours later, than its appulse with the main or central current. For a knowledge of this peculiar conformation of the November meteoric stream, as exhibited in its occurrences of the last few years, I am indebted to a very important observation communicated to me at the beginning of the present year by Mr. Benjamin V. Marsh, of Philadelphia, U.S., of the occurrence of a remarkably brilliant meteoric shower in China, on the morning of the 15th of November, 1867, *about twelve hours later, in absolute time*, than the beginning and end of the great display seen *in the same year* in the United States.* The times of the previously recorded star-showers, and of that conspicuously seen in America last year,

* *Proceedings of the American Philosophical Society*, vol. x. p. 384.

are thus graphically exhibited by Mr. Marsh in a single diagram (No. 1), which shows the dates and hours of their occurrence, in Philadelphia time, reduced to the common epoch of the year 1868.

Diagram No. 1.

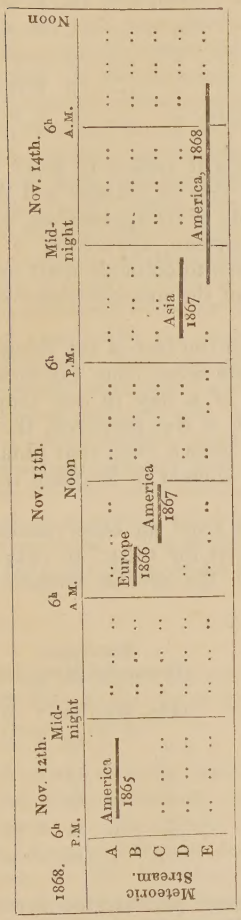
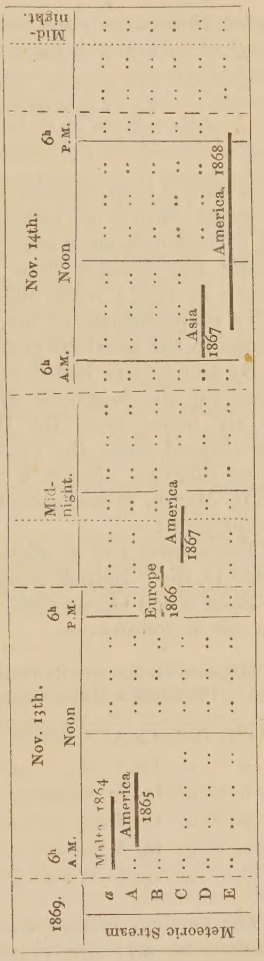


Diagram No. 2.



Place, Year, and Local Time of Maximum.						Philadelphia Time of Dit'o.					
				h	m					h	m
Shower A, Philadelphia,	1865,	Nov.	13,	3	0 A.M.	1865,	Nov.	13,	3	0 A.M.
B, London,	1866,	,,	14,	1	15 ,,	1866,	,,	13,	3	0 P.M.
C, Philadelphia,	1867,	,,	14,	4	30 ,,	1867,	,,	14,	4	30 A.M.
D, Shanghai,	67,	,,	15,	4	30 ,,	67,	,,	14,	3	30 P.M.
E, Philadelphia,	1868,	,,	14,	3	0 ,,	1868,	,,	14,	3	0 A.M.

Philadelphia should have crossed those streams in 1868 (as shown in the diagram), at the following times: —

	Meteoric Stream and its Duration. (Philadelphia Time).		Maximum. (Philadelphia Time).
	h m	h m	h m
A,	1868, Nov. 12, 7 0 P.M.	to 11 0 P.M.	1868, Nov. 12, 9 0 P.M.
B,	,, ,, 13, 7 0 A.M.	,, 9 0 A.M.	,, ,, 13, 8 0 A.M.
C,	,, ,, 13, 9 0	,, ,, 12 0 Noon	,, ,, 13, 10 30 ,,
D,	,, ,, 13, 7 30 P.M.	,, 11 30 P.M.	,, ,, 13, 9 30 P.M.
E,	,, ,, 13, 10 0 P.M.	,, 8 0 A.M. (Nov. 14)	,, ,, 14, 3 0 A.M.

A simple inspection of the diagram will suffice to show that the great shower of meteors seen last year in Europe and America corresponds more nearly, in the time of its appearance, *to the Asiatic star-shower*, than to the American apparition of the meteors in 1867; while the latter appears to coincide most nearly with a reappearance of the main body of the meteors observed in Europe in the previous year.

By adding five hours to the times in the last of the foregoing tables, for the longitude of Philadelphia west from Greenwich, and six hours more for the fraction of a day in one tropical year elapsed since the epoch for which it was prepared, the following diagram (No. 2, p. 7) is easily drawn from that obtained by Mr. Marsh, to aid observers of the shower in England in the present year: —

The shower (*a*) is added to the list from an observation recorded in the "British Association Report for 1865," p. 122; and the times when Greenwich should cross the several streams this year are as follows: —

	Meteoric Stream and its Duration. (Greenwich Mean Time).		Maximum. (Greenwich Mean Time).
	h m	h m	h m
a,	1869, Nov. 13, 5 0 A.M.	to 9 0 A.M.	1869, Nov. 13, 7 0 A.M.
A,	,, ,, 13, 6 0 ,,	,, 10 0 ,,	,, ,, 13, 8 0 ,,
B,	,, ,, 13, 6 0 P.M.	,, 8 0 P.M.	,, ,, 13, 7 0 P.M.
C,	,, ,, 13, 8 0 ,,	,, 11 0 ,,	,, ,, 13, 9 30 ,,
D,	,, ,, 14, 6 30 A.M.	,, 10 30 A.M.	,, ,, 14, 8 30 A.M.
E,	,, ,, 14, 9 0 A.M.	,, 7 0 P.M.	,, ,, 14, 2 0 P.M.

The vertical broken lines in the figure indicate the duration of darkness, and the vertical dotted lines the time when the radiant-point in *Leo* rises above the horizon at Greenwich. Should the main body of the meteors reappear in the present year, without having undergone any perturbations since November 1867, a few meteors of their display will be visible at Greenwich in the last hour preceding midnight on Saturday the 13th instant. If, on the other hand, the same gradual advance of the shower continues, which is perceived on the diagrams in all the three branches of which the stream consists, the reappearance of

the central shower will commence at Greenwich on Saturday at midnight, and continue until Sunday morning, at 3^h A.M.; the circumstances in this case being as favourable for its complete observation as they were on the night of the 13th–14th of November, 1866.

Although it does not appear that the outlying currents of the group are this year favourably situated for observation in Great Britain, yet the observation of a frequency of meteors towards daybreak on the morning of the 13th (Saturday) and of an unusual number of meteors after 11 o'clock P.M. on the night of the 14th (Sunday) would confirm the probable existence of two companion streams of the central meteor-current, and would afford material data for determining the limits of their extent.

The large meteor which was very generally seen in the south of England at about $6^h\ 50^m$ P.M. on the evening of the 6th instant, was also seen at Hawkhurst, in Kent, and the position of the luminous streak which it left visible, standing, apparently, quite perpendicularly over the point where the meteor disappeared, was noted exactly by the stars. It extended from *Vega Lyræ* to a point two or three degrees above and to the right of the star *μ Herculis*, where the meteor burst into fragments of white blue and orange colours, and left a small cloud of light at that spot, like a nebula, which remained visible for several minutes. The observer particularly noticed the extreme rapidity of the meteor's flight, and its perpendicular direction towards the Earth; but no sound of a report was heard, although he listened attentively for some minutes. The direction of the meteor was nearly due west, and its point of disappearance was about 40° from the horizon.

Note on the Sun's Motion in Space and on the relative Distances of the Fixed Stars of various Magnitudes. By Richard A. Proctor, B.A.

Having recently had occasion to examine Mr. Main's Table of the Proper Motions of 1167 Stars, and the conclusions with reference to the Sun's motion deduced from that table by a method devised by the Astronomer Royal, and carried out at his request by Mr. Dunkin, I have been led to notice certain facts which seem to me to be not without significance.

In the first place, I would call attention to the table drawn up by Mr. Dunkin, in which the sums of the squares of the stars' proper motions are compared with the corresponding sums when the proper motion of each star has been corrected for the Sun's calculated motion in space. Mr. Dunkin comments on the singular smallness of the correction thus introduced. And this view seems abundantly verified by the table, which runs as follows, the divisions 1 to 7 corresponding to Struve's arrangement of the stars in order of magnitude:—

Sums of Squares of Motion in Parallel.

	Uncorrected.	Corrected.
Division 1	2·0637	1·2123
2	1·8743	1·9292
3	9·2894	9·5607
4	6·4732	5·4608
5	43·4126	42·4236
6	14·8637	14·2750
7	0·7814	0·7215
Sum	78·7583	75·5831

ums of Squares of Motion in N.P.D.

	Uncorrected.	Corrected.
Division 1	6·7231	5·6883
2	0·5351	0·7805
3	4·9739	4·7569
4	6·9390	6·5255
5	39·4335	38·7292
6	4·5671	4·3376
7	0·0951	0·0904
Sum	63·2668	60·9084

Commenting on this result, Sir John Herschel remarks:—"No one need be surprised at this. If the Sun move in space, why not also the stars? And if so, it would be manifestly absurd to expect that any movement could be assigned to the Sun by any system of calculation which would account for more than a very small portion of the totality of the observed displacements."

It had always seemed to me that this conclusion might require to be modified if the question were subjected to mathematical scrutiny; my reason for forming this view being this,—that the largeness of the number of stars operates as much to increase the extent of the correction as to increase the amount of the uncorrected squares, since every star is affected by the Sun's motion in space.

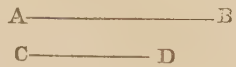
It occurred to me recently that the following simple geometrical proof serves to show that the correction to be looked for is much larger than that which Mr. Dunkin's figures exhibit.

Suppose that in any small region of the sky there are a large number of stars (say n), all at the same distance from the Earth, and travelling in all directions with a velocity exactly equal to that with which the Sun is travelling. Let us suppose that the motion of any one of these stars which is travelling at right angles to the line of sight carries the star over an arc p in a year; and that the region of the sky is so situated that the effect of the

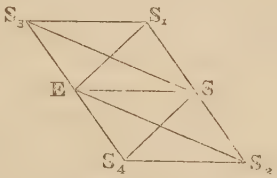
Sun's motion on a star at rest (at the given distance) would be to produce an apparent annual proper motion q'.

Let A B (fig. 1) $= p$
C D $= q'$

Fig1



Suppose two of the stars to move in opposite directions, S S_1, S S_2, along the same straight line, both (we may suppose for convenience) starting from S, and the lines S S_1, S S_2, being in reality equal to A B, but foreshortened. Then the sum of squares for these stars, if unaffected by the Sun's motion, would be



$$= S S_1^2 + S S_2^2,$$

where S S_1, S S_2, represent the *apparent* lengths of these lines.

Suppose that S E is the apparent motion due to the Sun's real motion, so that S E = C D.

Complete the parallelograms S S_1 S_3 E and S E S_4 S_2, and draw their diagonals. Then the sum of squares for the two stars as affected by the Sun's motion,

$$= S S_3^2 + S S_4^2$$
$$= S_2 E^2 + S_1 E^2$$
$$= S S_1^2 + S S_2^2 + 2 E S^2$$

Therefore, for every pair of stars moving in opposite directions, there is an increase of $2 q'^2$ in the sum of squares. Therefore there is an average increase of q'^2 for each star in the region.

Now a moment's consideration will show that, precisely as the lines S S_1 and S S_2 vary from the full length A B to zero, as we vary the supposed motion in all directions around S (for stars in a given small region of the heavens), so does C D vary from A B to zero as we shift the small region over the heavens. It is not merely that the limits of change are the same, but the proportion of lines of a given length S S_1 for the star's motion is exactly the same as the proportion of corrections equal to S S_1 for the Sun's motion. Since, therefore, for each region there is an increase per star equal to the square of the proper motion due to the Sun's motion (for that region), and the several stars of each region have proper motions varying according to exactly the same law as that according to which the effect of the Sun's motion varies over the celestial sphere, it is obvious that for the whole sphere the effect of the Sun's motion must be to increase the sum of squares by an amount exactly equal to the sum of squares due to the stars' own motions. In other words, the sum of squares is doubled

through the effect of the Sun's motion, if only the Sun be assumed to travel at the same rate through space as the several stars at a given distance from him.

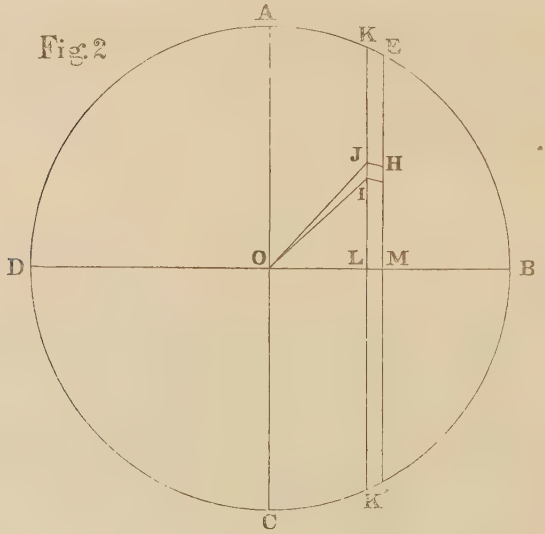
It is obvious also that the same is true if the stars at a given distance move with different velocities, but the Sun's velocity is the mean (its square equal to the mean of sum of squares) of the velocities of the stars at said distance.

Further, if the mean velocity of the stars at a given distance be p, and the Sun's velocity be q, then we have the ratio

$$\frac{\text{Corrected sum of squares}}{\text{Uncorrected sum}} = \frac{p^2}{p^2 + q^2}.$$

Before proceeding to apply this law, I propose to deduce it by another process, which will serve to indicate what is the actual sum of squares, corrected or uncorrected, for a given large number of stars, all at the same distance from the Earth.

Fig. 2



Let A B C D, fig. 2, represent a small circular space on the celestial sphere; and suppose a large number of stars within this space, travelling in all directions with a mean velocity which would give to one moving at right angles to the line of sight an annual proper motion $= p$. Then, in order to determine the mean square of the apparent proper motion — the Earth being supposed to be at rest — we may suppose every star to start from O, O B $= p$, and the points towards which the stars are severally moving, uniformly spread over the sphere A B C D.

Take now a thin zone of the sphere's surface by parallel planes perpendicular to O B, through L K, M E, where

$$O L = x, \qquad O M = x + \delta x.$$

Then the number of stars whose directions lie towards some part of this zone, is (by a well-known property of the sphere),

$$= \frac{n \cdot \delta x}{2p},$$

n being the total number of stars.

Again, take planes B O I, B O J, inclined at angles θ and $(\theta + \delta\theta)$ to the plane through O B and the observer's eye.

Then the number of stars, whose direction is towards some part of the small element I H of the sphere's surface,

$$= \frac{n \delta x}{2p} \cdot \frac{\delta\theta}{2\pi}.$$

When the element I H is taken small enough, each of these stars has an apparent proper motion, whose square

$$= x^2 + (p^2 - x^2) \sin^2 \theta.$$

Therefore, it follows that the mean value required

$$= \frac{1}{p\pi} \int_0^{\frac{\pi}{2}} \int_{-p}^{+p} \left\{ x^2 + (p^2 - x^2) \sin^2 \theta \right\} d\theta \, dx$$

$$= \frac{2p^2}{\pi} \int_0^{\frac{\pi}{2}} \left(\frac{\cos^2 \theta}{3} + \sin^2 \theta \right) d\theta$$

$$= \frac{2p^2}{3\pi} \int_0^{\frac{\pi}{2}} \left\{ \frac{1 + \cos 2\theta}{2} + \frac{3(1 - \cos 2\theta)}{2} \right\} \delta\theta$$

$$= \frac{2p^2}{3}; \qquad (1)$$

and the sum of squares is therefore

$$= \frac{2np^2}{3}. \qquad (2)$$

Next let us inquire what the sum would be if the solar system were moving in such a manner that each star within the circle A B C D was affected by a proper motion q (in any direction) due to the Sun's motion.

Let the motion be parallel to O B, and from right to left. Then it is clear that the only change in the expression to be integrated, is, that for x^2 we must write $(x - q)^2$. Therefore the mean square will be

$$\frac{2p^2}{3} + \frac{1}{p\pi}\int_0^{\frac{\pi}{2}}\int_{-p}^{+p} (q^2 - 2qx)\, d\theta\, dx$$

$$= \frac{2p^2}{3} + q^2. \qquad (3)$$

In this case, then, the sum of squares is

$$= \frac{2np^2}{3} + nq^2. \qquad (4)$$

Lastly, we have to inquire what is the sum of squares for N stars scattered over the whole of the celestial sphere at a given distance R, each affected by the proper motion p *in space*, the Sun being affected by a motion P (p and P representing the arc-motions due to these respective proper motions, when supposed to be taking place in a direction at right angles to the line of sight, and to be seen from a distance R).

In fig. 2, let A B C D now represent the celestial sphere, and take

$$O L = y$$
$$L M = \delta y;$$

then the effect of the Sun's motion upon all stars in the zone KMK′ will be to affect them with a proper motion,

$$P \sin \cos^{-1}\left(\frac{y}{R}\right)$$

$$= P\sqrt{\left(1 - \frac{y^2}{R^2}\right)}.$$

Hence, by (2) the average proper motion of stars in this band

$$= \frac{2p^2}{3} + P^2\left(1 - \frac{y^2}{R^2}\right);$$

and, therefore, for the whole sphere, the average proper motion,

$$= \frac{1}{2R}\int_{-R}^{+R} \left\{\frac{2p^2}{3} + P^2\left(1 - \frac{y^2}{R^2}\right)\right\} dy$$

$$= \frac{2p^2}{3} + P^2 - \frac{P^2}{3}$$

$$= \frac{2}{3}(p^2 + P^2). \qquad (5)$$

and the sum of squares of proper motions

$$= \frac{2\,N}{3}\,(p^2 + P^2) \qquad (6)$$

If $P = p$ (that is, if the Sun's motion be assumed equal to the average motions of the stars) the sum of squares,

$$= \frac{4\,N}{3} \cdot p^2. \qquad (7)$$

The use of the integral calculus, as above, would only be justified where N is infinite; but where N is considerable, the result must be a close approximation to the truth, with the assumed conditions. And even where N is small, the above result is the most probable, in the case of a random distribution of the N stars, and of the directions of their motion.

Now, if we apply these results to the tables given above, we can determine how far the observed proper motions of the stars are consistent with the supposition that the Sun's proper motion is not very different from the mean proper motion of the stars of different magnitudes; and thence we can form an opinion as to the justice of those estimates of the stars' distances on which the values of the corrections tabulated above have been determined.

First, we require the sums of squares of proper motions, without reference to direction. Since the square of a star's proper motion is equal to the sum of the squares of the star's proper motion in parallel and in N.P.D., we have only to add the respective sums of squares in the two tables given above, to deduce the following table:—

Sums of Squares of full Proper Motion.

	Uncorrected.	Corrected.	N.
Division 1	8·7868	6·9006	9
2	2·4094	2·7097	55
3	14·2633	14·3178	146
4	13·4122	11·9863	238
5	82·8461	81·1528	330
6	19·4308	18·6126	368
7	0·8765	0·8119	21
Sum	142·0251	136·4917	1167

Let us first apply formulæ (2) and (6), (remembering that formula (2) is true for the whole celestial sphere). We have from them,—

$$\text{Uncorrected sum of squares} = \frac{2\,N}{3}\,(p^2 + P^2),$$

$$\text{Corrected sum of squares} = \frac{2\,N \cdot p^2}{3}.$$

Hence

$$P^2 = p^2 \left(\begin{matrix} \text{correction} \\ \text{corrected sum} \end{matrix} \right).$$

Applying this successively to the several divisions, we obtain

For Division 1	$P = p$ (·52)
2	P has an imaginary value
3	P has an imaginary value
4	$P = p$ (·34)
5	$P = p$ (·18)
6	$P = p$ (·21)
7	$P = p$ (·28)

The results for the second and third divisions suffice to show that the numbers of stars which fall under these heads are insufficient for a satisfactory determination; and therefore, *à fortiori*, the number of stars in division 1 is insufficient. Hence the result $P = p$ (·52) must be dismissed as valueless. But even at this stage of the inquiry we begin to recognise that there must be something wrong about our assumptions. For even if the numbers 55 and 146 were not in themselves large enough to lead us to expect a satisfactory evaluation for stars in the second and third divisions, yet the fact that the Sun's motion in space has been correctly deduced from a smaller number of stars would justify such an expectation.

I should be led then to suspect from this evidence, that if divisions 1, 2, and 3, had been taken together, a more satisfactory conclusion would have been arrived at, notwithstanding the apparent necessity of assigning different distances to stars in these divisions. However, it is not possible to determine how far this suspicion is justified without going over Mr. Dunkin's labours afresh, with changed assumptions; and I have no leisure for the long processes of calculation this would involve.

Next we may notice that the results for divisions 4, 5, 6, and 7, are very far from satisfactory. I cannot think it credible that the real solution of the difficulty involved in the smallness of the observed corrections is to be found in the assumption that the mean motion of stars of the fourth magnitude is three times as great as the Sun's, the mean motion of fifth-magnitude stars nearly six times as great as the Sun's, and so on.

If we try the effect of diminishing the assumed distances of stars of the 4th, 5th, and 6th divisions, so as to accord with the observed relations, on the assumption that in reality $P = p$, we have (the assumed distances being 3·76, 5·44, and 7·86,* respectively) the following results:—

* Struve's values.

For Division 4	Distance = 3·76 × ·34 = 1·28
5	,, = 5·44 × ·18 = 0·98
6	,, = 7·86 × ·21 = 1·65

As division 7 includes but 21 stars, we cannot expect any trustworthy results from treating it in the same way.

The evidence thus far seems strongly opposed to accepted views respecting the distances of stars of the smaller visible magnitudes. A further inquiry on this point seems, therefore, suggested. And it is obvious that in the above table of sums of squares we have the means of testing how far the assumed distances of the stars of various orders accord with the observed proper motions. I make the assumption that the Sun's motion is equal to the average proper motion of the fixed stars. This assumption affects the actual, but not the relative, values of the distances which result from the following processes.

We have then formula (7) to deal with. Applying it to the successive orders of stars, *i.e.* putting $\frac{4}{3} N p^2$ successively equal to the values tabulated in the foregoing columns of uncorrected sums of squares, we obtain,

	Apparent Proper Motion.	Resulting Distance.
Division 1	″0·857	1
2	0·182	4·7
3	0·268	3·2
4	0·208	4·1
5	0·433	2·0
6	0·191	4·5
7	0·173	5·0

This result is unsatisfactory in the extreme. We find stars of the second magnitude placed (according to this mode of estimating their distances) further from us than stars of the 3rd, 4th, 5th, and 6th magnitudes.

Remembering the evidence we have already had, that (1) there is something erroneous in our assumptions respecting star-distances; and secondly, that small numbers of stars are insufficient for our guidance, let us apply a test which there ought to be no mistaking. Let us divide the stars into two sets, the first including divisions 1, 2, 3; the second, the remaining divisions and let us apply formula (7) to these sets.

We obtain,

For Set 1, Apparent Proper Motion,

$$= \sqrt{\frac{3}{4}\left(\frac{25'4595}{210}\right)} = 0''{\cdot}3015,$$

For Set 2, Apparent Proper Motion

$$= \sqrt{\frac{3}{4}\left(\frac{116{\cdot}5656}{957}\right)} = 0''{\cdot}3022.$$

This result would make the mean distance of stars of the first three magnitudes equal to (or *very slightly less* than) the mean distance of stars of the next three magnitudes!

I am very far from supposing that this result accurately represents the relations subsisting among the stars; but I do think that it suffices to render the usually accepted views respecting stellar distribution wholly untenable. Remembering that whatever theory we form regarding the relation between the apparent brilliancy and the real distance of the stars, we must yet recognise the fact that the stars are at very various distances from us. I think it must be admitted that the apparent brightness of a star is, to a certain extent, an argument of relative proximity. A large proper motion is also an argument of relative proximity. If the two indications agreed either for separate stars or for sets of stars, arranged according to apparent brilliancy, there would be no difficulty. As we find, however, that there is no such agreement, we are forced to consider whether brightness or large apparent motion is the stronger evidence of proximity. Judging from the analogy of the solar system, in which the range in the variations of magnitude is enormously greater than the range in the variations of velocity, we seem strongly led to look on the proper motions of stars as in reality the best evidence we have respecting their distances. But this conclusion is very much strengthened when we remember that the dynamical conditions in the sidereal system must be much more unfavourable to the occurrence of wide variations of velocity, than the conditions which prevail in a system of bodies circling around a central body enormously large compared with any of its dependent orbs.

I think, then, that I may fairly look upon the above inquiry as affording very striking evidence in favour of the view I had formed from other considerations, that the assumed estimate of the distances of the smaller stars has been greatly overrated. And as this conclusion may obviously be extended to yet smaller stars, I think that I have not been deceived in looking upon the relations which subsist between the Milky Way and the lucid stars in its neighbourhood as very much more intimate than has been commonly supposed. I believe that future researches will prove, not only that the Milky Way as a whole is much nearer than we have been imagining, but that portions of it are absolutely nearer to us than the brightest of the single stars. That parts of the Milky Way, for instance, in the neighbourhood of α *Centauri* (the nearest of the fixed stars, so far as is yet known), are nearer to us than that star, I think the whole aspect of the galaxy in that neighbourhood suffices to suggest, if not to demonstrate.

Suggestion of a Method of Imitating the Transit of a Planet over the Sun. By the Rev. T. R. Robinson, D.D., F.R.A.S., &c.

While reading Mr. Stone's admirable papers on the transit of *Venus*, it occurred to me that experimental information as to the "black drop," which plays so important a part in this inquiry, may be obtained by means which I formerly used in some researches on irradiation, that were published in the 5th volume of the Society's *Memoirs*.

To obtain an artificial Sun I fixed a plate of brass, in which was a small circular aperture, in the focus of a good telescope. This was viewed, collimator-wise, with another telescope; and when illuminated by a lamp placed behind it, appeared as a luminous disk, 17′ diameter, sharply defined, and about as bright as the Sun was in the transit instrument. By interposing a piece of oiled paper the disk became much fainter, though the wires in the micrometer of the observing telescope were quite distinct. Making them internal tangents to the disk, and withdrawing the oiled paper, the limbs projected outside the wires from two to three seconds by the effect of irradiation.

Now an artificial planet may be obtained in two ways. 1. A small opaque disk may be carried by the frame of a micrometer in the observing telescope; it will appear as a black spot on the luminous disk, which can be moved at pleasure. Making the illumination of the aperture as faint as is consistent with distinct vision, the circumference can be brought into *real* contact and the corresponding micrometer reading noted. Then, using the full illumination, I expect that the "black drop" would appear, and that an *apparent* internal contact could also be produced. If the arm carrying the opaque disk be supposed likely to interfere with an exact appreciation of the phenomena, it might be supported by a central perpendicular wire an inch or two above the micrometer frame.

2. Or, which seems preferable, the opaque disk may be placed within the aperture in its plane, and moveable in that plane by a micrometer attached to the collimating telescope. This plan has the advantage that both Sun and planet are formed under precisely similar optical conditions, and that the apparent motion of the latter can be more conveniently imitated by attaching a driving-clock to the micrometer-screw.

The precautions which were required to give the aperture an edge which can bear high magnifying, are described in the paper referred to, and they would probably suffice to give an exact metal disk. I would, however, prefer one formed by drilling a conical hole through two pieces of glass cemented together, as described for the aperture; on separating them the edges of the sections which had been in contact would be as smooth as art can make them, and by filling one of them with black cement, a true

disk would be obtained, which could be placed exactly in the plane of the sun-aperture. The glass should be attached to the micrometer-frame, and thus no disturbance of the images by any influence of a support could occur.

If the illumination were made by magnesian or electric light, this apparatus would be also available for Photographic experiments on the Transit.

On the Increase of Probable Errors in a Transit of Venus as dependent upon the Smallness of Normal Velocity. By E. J. Stone, Esq.

In the *Monthly Notices* for April there appears a short paper of mine, "On the Comparative Clinging of the Limbs of *Venus* and the Sun in the Transit of 1874," and some consequences which appeared to follow therefrom.

In discussing the observations made in 1769, I convinced myself that the chief source of error to be feared was erroneous assumption of semidiameter. The errors arising from causes which chiefly affect ordinary transit-observations, such as errors in subdividing seconds of time, loss of time in mechanically registering impressions conveyed through the eye to the mind, &c. sink into insignificance compared with the three or four seconds which we had to allow for the probable error of a contact observation.

In observations of this kind the observers have to seize some well-marked phases of the gradual destruction of, and restoration of, the irradiation near the point of contact of the Sun's limb. The appearances are complicated by considerations of atmospheric disturbances, aperture of telescope employed, density of absorbing medium made use of to reduce the Sun's glare, eye of the observer, defining character of the telescope, and the power employed. But, under given conditions, the phenomenon — as, for instance, real contact or total disappearance of connecting ligament — will take place with a definite angular separation between the limbs.

If the attention is drawn to the first appearance of, or disappearance of, the connecting ligament, then that ligament must have a definite breadth, in order that, under the given circumstances of observation, it may be visible by contrast upon the Sun's disk. The question of seeing the ligament is not a question of sufficient time to enable us to pick it up, but of sufficient optical means to distinguish it. Such, at least, was and is my own opinion.

In the transit of *Mercury*, 1868, November, Mr. Carpenter, observing at Greenwich with a small telescope and low power, about 90, states that "the black ligament appeared to form *in-*

stantly, and to be of the breadth indicated, about $\frac{1}{3}$ the diameter of the planet." Now, although Mr. Carpenter observed the first formation, to him, of the black ligament, his time is about 13^s later than the time given by Mr. Lynn, observing with a power of about 170, and with a much better instrument. There was here plenty of time for Mr. Carpenter to see the ligament within the 13^s which elapsed between Mr. Lynn's observation and his own, but he did not see it, because, I maintain, his instrumental means were insufficient to appreciate the existence of the ligament by contrast, until it had become of a certain definite breadth. If this be the case, he would not have seen the ligament sensibly sooner had a period of 24^s elapsed whilst *Mercury* was moving over the distance from its position corresponding to Mr. Lynn's observations to that which it had at the time of his own observations.

The errors of such observations really arise when we assume that the phase of the phenomenon observed by one observer corresponds to the phase observed by another observer in such a way that each takes place at the same distance between the centres. The error thus committed is measured by the deviation of the true distance between the centres at which the observation was really made, and the mean distance for all the observations of apparently similar phenomena which are to be combined. The errors, therefore, measured in time, must be inversely proportional to the normal velocities. Another illustration, with a practical bearing, may be given, as follows:—Suppose that photography should be applied to the transit of *Venus*, by obtaining a great many photographs, by some quick process, whilst *Venus* is on and near the limbs, at points as near the points of maximum effect as possible. Then, with such a process, the errors in measuring the angular distances between the centres will be sensibly independent of the relative motions of *Venus* and the Sun; but if we should wish to apply these results to the determination of solar parallax by referring back all the photographs to some definite distance from the Sun's centre for comparison with photographs made at other stations, then, in referring these results back and determining the *time* at which *Venus* was at some definite distance from the Sun's centre, we should commit errors in *time* in proportion to the slowness of normal velocity.

These views led me to infer that the probable error of every single observation made in 1874 must be considered as injuriously affected by the slow normal velocity, and I gave in my April note the ratio of the mean probable errors arising from this cause in the transits of 1874 and 1882. I can understand differences of opinion existing with respect to the accuracy of these views; but I must confess my inability to find any full consideration of such views brought forward in the March number of the *Monthly Notices*.

On some Attempts to render the Luminous Prominences of the Sun visible without the use of the Spectroscope. By Warren De La Rue.

The progress of scientific discovery may be promoted by the record of one's failures, for it tends to prevent the same paths from being trodden by future explorers; on this account I venture to lay before the Society a description of some experiments on which I have been engaged since my return to Cranford in the spring of this year. Although these experiments have hitherto led to no satisfactory result, I am still pursuing them by varying the form of apparatus and the substances employed, in the hope of ultimately attaining the object in view. Notwithstanding the great success which has attended the study of the luminous prominences by Janssen, Lockyer, Huggins, Secchi, and more recently by Zöllner, it will be conceded that it is extremely desirable to find means of viewing these entities in their true position around, and on the Sun, without being compelled to distort the Sun's surface by spreading it out into an elongated sheet of light.

It must have undoubtedly suggested itself to many astronomers, that since the prominences emit light of but a few distinct and definite refrangibilities, it may be possible to absorb or to reflect out of the field of view the greater portion of all the other rays of the spectrum emanating from the Sun, by intercepting the light collected by an object-glass or mirror, by appropriate media. Indeed, the proposal of Huggins to combine the employment of the spectroscope with the use of absorbing media, naturally suggests the question,—why use the spectroscope at all?

The plan which I proposed to myself was twofold: to attack the F line on the one hand, and the C line on the other. In order to see the prominences by means of the F or green line, I adopted the following plan:—If the reader will suppose that in the case of a Newtonian, a right-angled prism is employed in lieu of the diagonal reflector, it will be evident that while the function of the hypothenuse is to reflect in totality the light it receives from the concave mirror, the two other sides of the prism are available for other objects, and may be covered by certain media through which the light must pass, and by which it may be altered in character by the reflection and dispersion into space of part of the rays, and the transmission to focus of others. The most promising covering medium for the right-angled surfaces of the prism appeared to me to be metallic gold, and fortunately the researches of Wernicke have furnished a means of obtaining a deposit of gold as brilliant and firmly adhering as the well-known silver deposits. The deposit of gold transmits a beautiful green, which includes the F line; and the Sun may be viewed without the slightest inconvenience after the light has passed through two films sufficiently thick. One advantage in gilding two surfaces of the prism consists in precluding the probability of any unaltered light from reaching the eye, for if minute holes occur in one film

it is not likely that others exist in the second film precisely opposite to them. The Sun's image when viewed through the gold films is beautifully defined, and perfectly cool, and very agreeable to the eye ; indeed, if the hand be held in the principal focus of a 13-inch mirror, after the light has passed through the gold films, only a slight sensation of warmth is experienced; whereas a piece of wood, similarly placed, when the light is simply reflected by metallic mirrors or a prism not gilded, would soon be ignited : the gold film evidently, therefore, excludes most of the heat rays. Notwithstanding the hopes I entertained of rendering the prominences visible, I have not yet been able to succeed. There is, however, one circumstance which I ought to mention, because it may have contributed to the failure : it is this, it is extremely difficult to procure prisms in which the plane of the hypothenuse of the right-angled prism is truly parallel to the edge formed by the two sides at right angles. Very nearly true they are, but not perfectly so. In consequence of this imperfection, a great number of images of the Sun are seen (on account of a series of internal reflections), when the prism is held in the direction of the Sun; and although these may not overlap each other in the telescope, yet they cause an amount of diffused light which materially interferes with the possibility of seeing the prominences. The most obvious remedy is, of course, to try to procure a true prism ; but the Astronomer Royal, to whom I imparted my difficulty, has suggested passing the light through pairs of prisms of small angle, superposed so as to form an optical monad, which would act as a parallel plate of glass, but in which the component prisms offer four sides which may be gilded. If one monad is not sufficient to screen away all the useless rays, then a second or a third might be used.

It may be mentioned that, in the case of the reflector, I have used a second smaller right-angled prism with two faces gilded, and placed between the eye-piece and the first prism before mentioned, and that the Sun's image was still very bright after passing through four films of gold. Also that I have employed a gilt prism with my 4-inch Dallmeyer.

It is not impossible that I may succeed in obtaining a photographic image of the prominences by using the gilt prism, even though I may not succeed in seeing them. Before, however, abandoning the attempt of rendering them visible by means of the gold films, I am having made sets of prisms, as proposed by Mr. Airy, and shall also endeavour to obtain true right-angled prisms ; unfortunately, the Sun's low altitude will interfere for some months with the prosecution of the experiments.

Now, with regard to the C or red line, my plan of attack has been to employ red fluids, placed either between the eye-piece and mirror or object-glass, as the case may be, or between the eye-piece and eye. The two as yet experimented on without result are, a solution of carmine in ammonia, and a solution of aniline red in alcohol.

It is, of course, possible to ascertain beforehand whether the C ray is transmitted by the fluid, provided a suitable spectroscope is used; a trial showed that the red light transmitted by a solution of carmine in ammonia included the F ray, but this solution did not bring to view the luminous prominences. I am now preparing some pure carminic acid and some compounds of that acid for future experiments, and I am having a series of bottles made of larger capacity than those I have hitherto employed; for I have found that unless the light passes through one or two inches, or even more, of some of the coloured fluids, the eye is inconvenienced by the great brilliancy of the Sun's image. The vessels used have, of course, two sides of parallel glass.

On the Floor of Plato. By W. R. Birt.

During the last forty-eight years, occasional notices of the spots and marking on the floor of the walled plain *Plato* have appeared. In consequence of having given considerable attention to *Plato* and its surroundings in the years 1860 to 1863, I collected all the observations of the spots that I became acquainted with, numbering in the whole fifty-six, having reference to fourteen spots, two or three of which had been observed as craters, one being double. The greatest number observed *simultaneously* was seven, by Gruithuisen, in the year 1825. On the 23d of February of the present year, Mr. Pratt, of Brighton, observed with his 8-inch silvered-glass reflector eleven spots at the same time. They were not, however, eleven of the spots which had been observed previously, but included four unrecorded and a second double spot. Mr. Pratt has steadily continued his observations up to the present time, and determined the relative positions of twelve spots by alignment. Mr. Edward Crossley, of Halifax, has also kindly requested his assistant, Mr. Joseph Gledhill, to make continuous observations on them with his 9·3-inch achromatic by Cooke. The observations made by Mr. Gledhill and Mr. Pratt I have regularly received. The following are the names of the astronomers who have observed the floor of *Plato*. Gruithuisen, Mädler, Challis, Knott, the late Lord Rosse, the late Rev. W. R. Dawes, Baxendell, Dr. Dobie, Birt, Pratt, Crossley, Gledhill, and Elger, the number of observations being 297, in fifty series, and the number of spots observed twenty-five, including the companion of Dawes's double spot. In the annexed Table the number of each spot is given in accordance with the accompanying diagram, the name of the discoverer, the number of times each spot has been observed since the commencement of the present year, 1869, the comparative degree of visibility, that of the central spot, No. 1, being reckoned as unity, or 1·00, and remarks on the positions and general characters of the spots.

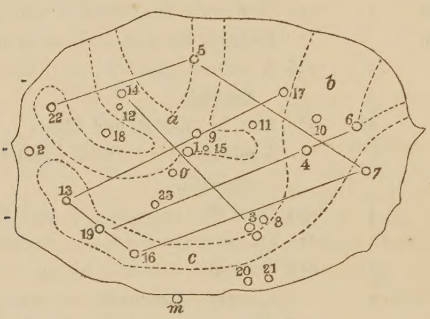
Spots on Plato.

No.	Discovered by	Obs.	Visibility.	Remarks.
0	Gruithuisen			N.W. of 1, apparently nearer than 23.
1	Gruithuisen	32	1·000	Central, easy, generally of the same appearance.
2	Gruithuisen	1		Near the W. rim, seen once only by Gledhill.
3	Gruithuisen	23	·782	Dawes's double crater, frequently seen single.
4	Gruithuisen	27	·844	Departs often from typical state; seen double by Pratt.
5	Challis	20	·625	Not seen so frequently as 3.
6	Gruithuisen	8	·250	Seen lately by Pratt and Gledhill.
7	Gruithuisen	8	·250	Seen lately by Gledhill, Pratt, and Elger.
8	Gruithuisen			Probably the S.E. companion of 3.
9	Dobie	5	·156	A minute spot S.E. of 1, seen by Dawes and Gledhill.
10	Pratt	6	·187	Seen by Gledhill and Elger.
11		1		A minute spot between 9 and 17, seen once by Gledhill.
12	Gledhill	3	·094	N. of No. 14; seen only by Gledhill.
13	Pratt	17	·531	On curved streak c, now frequently seen.
14	Pratt	21	·656	On central arm of "trident," frequently seen.
15	Dawes	1		A minute spot due E. of 1, seen once by Gledhill.
16	Pratt	13	·406	On curved streak c, frequently seen.
17	Mädler	21	·656	On wedge of "sector," frequently visible.
18	Gledhill	1		Near N.W. arm of "trident," alignment 1 : 18 : 22.
19	Gledhill	17	·531	On curved streak c, frequently seen.
20	Knott	3	·094	Near N. border, probably seen by Gledhill.
21	Knott			E. of 20, near N. border.
22	Mädler	9	·281	Seen lately by Gledhill, Pratt, and Elger.
23	Pratt	1		Alignment 19 : 23 : 4.

As the comparative visibility of a spot depends upon its size and brightness, the larger and brighter spots are more likely to be seen in indifferent states of the Earth's atmosphere than those which are small and of less brilliancy. The number of observations, 238, between Feb. 23 and Sept. 27, 1869, afford a preliminary basis for an approximate measure of the degree of visibility of each according to the number of times it has been observed. The present order of visibility is as under :—

No.	Visibility.	No.	Visibility.	No.	Visibility.	No.	Visibility.
1	1·000	17	·656	16	·406	10	·187
4	·844	5	·625	22	·281	9	·156
3	·782	13	·531	6	·250	12	·094
14	·656	19	·531	7	·250	20	·094

In most cases the degree of visibility is underrated, as it is probable that in another series by the same observers many of the smaller spots may be *looked* for and *seen* under circumstances which, in earlier observations, occasioned them to have been overlooked, unless specially sought for. The spots remaining in the same condition, affected only by variations in the Earth's atmosphere, the probability is that successive series of about the same number of observations by the same observers will give similar degrees of visibility. It may be remarked that on no occasion have all the spots been seen simultaneously; the greatest number recorded in one watch is thirteen, and these were not all seen at once.



The dotted lines in the diagram are intended to represent the probable boundaries of the lighter markings on the floor of *Plato*, which are by no means of a sharp or definite character, but ill-defined and passing gradually into the darker hue. Although there is some reason to conclude that they are really permanent, yet they are seldom seen alike. The observations of these delicate markings are not at present sufficiently numerous to allow of an examination into the circumstances that affect their visibility. In conclusion, I would remark that this communication is intended rather as a contribution towards elucidating a confessedly difficult branch of Selenography, than establishing the order of visibility of the spots above named, further observations being required for this object.

Lunar Eclipse of 23rd and 24th *July*, 1869. By John Tebbutt, Jun.

The lunar eclipse of the 23–24th July was observed here as follows:—

	Windsor M.T. d h m s
First contact with shadow	23 10 43 35
Last ,, ,, ...	13 29 28

The contacts were observed with a power of about 90 on my $3\frac{1}{4}$-inch refractor, but the results are by no means satisfactory, owing to the irregular and ill-defined character of the shadow.

The colour of the shadow was very dark iron grey; the red tint usually seen in total eclipses was not noticed. Even with a power of about 30 and the illuminated disk excluded from the field of view, the details on the obscured portion of the surface were perceived only with the greatest difficulty. The eclipsed limb was, however, pretty distinct, but it appeared to be an arc of a smaller circle than the illuminated limb, the bending inwards of the limb at the cusps being very perceptible.

Windsor, New South Wales, August 11, 1869.

Comet 1869.

Discovered October 11, at Vienna, by M. Tempel.

Mr. Hind, in a letter dated Twickenham, Nov. 17, addressed to the President, writes, that Dr. Winnecke having sent him an observation on October 18, which fits in very well with two in the *Astronomische Nachrichten*, he has calculated the following approximation (apparently a very fair one) to the elements of the orbit:—

Perihelion Passage	1869, Oct. 8·4421 G.M.T.	
Longitude of Perihelion	124° 41′·1	Apparent
,, Ascending Node ..	311 24·4	Equinox.
Inclination	68 48·8	
Log. perihelion distance	0·0895	

Motion retrograde.

He remarks that these elements do not resemble those of any comet previously computed. They appeared in "*Nature*," No. II.

Occultations of Stars, and Eclipse of Satellite of Jupiter, observed at Leyton. By C. G. Talmage.

1869, July 21.—Occultation, disappearance of 33 *Sagittarii*; power 70 on 10-inch refractor. G.M.T. $12^h\ 3^m\ 23^s{\cdot}92$.

Good, time exact, cloudy at reappearance.

1869, August 2.—Occultation, reappearance of α *Tauri*; power 70 on 10-inch. G.M.T. $13^h\ 13^m\ 48^s{\cdot}23$.

Came out instantaneously, no hanging on limb. Moon hid by a tree at disappearance.

1869, October 11.—Occultation, reappearance of 31 *Sagittarii* on Moon's bright limb; power 70 on 10-inch. G.M.T. $6^h\ 7^m\ 55^s{\cdot}93$.

It was too light for disappearance, as light clouds covered the Moon.

1869, October 11.—Eclipse, disappearance of *Jupiter's* II. Satellite. G.M.T. $8^h\ 50^m\ 37^s{\cdot}21$.

Excellent definition, time good.

Mr. Barclay's Observatory, Leyton, Essex,
Nov. 11, 1869.

Instrument for Sale.

A 30-inch Transit Instrument by Troughton and Simms, for mounting on stone; $2\frac{1}{2}$ in. object-glass. Declination circle, divided on silver, with two verniers, reading to $\frac{1}{2}$ min. Three eyepieces, 37, 66, and 100. Diagonal prism. Micrometer to eyepiece, with Level, complete in case. Price 35*l.*; cost 55*l.* Apply to L. P. Casella, 23 Hatton Garden, London, E. C.

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